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ARTICLE VII.

ON THE ORIGIN OF THE LUNAR DIVISION OF THE ZODIAC REPRESENTED IN THE NAKSHATRA SYSTEM OF THE HINDUS.

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Presented to the Society May 17th, 1865.

UPON presenting an essay on this subject, after so much has been written respecting it by the most learned men of Germany, France, Great Britain, and America, a few preliminary remarks seem called for by way of explanation. I had prepared an essay on the originality of the Hindu astronomy, to be appended to the Translation and Notes of the Sûrya-Siddhânta. But the work had become so much extended, that it was thought best by the Committee of Publication not to print it. A prominent object in that essay was, that I might express my views on some points in reference to which I had found myself at variance with Prof. Whitney—which views of my own, I being distant from the place of publication, had not found a place in the notes. The originality of the *nakshatras* in India was one of the topics I had prominently noticed. But I was allowed a limited space for a closing note to the Translation, in which I stated in brief the fact that I differed from Prof. Whitney in regard to some matters which he had treated in the notes, and mentioned what the principal points of difference were. This I did, as I then remarked, in order that I might feel free to revert to the subject at a future time. For reasons which I need not specify, I was prevented from doing this until after the appearance of Prof. Whitney's essay criticising the views of Weber and Biot on the *nakshatra* system, published in the Society's Journal (above, pp. 1–92). In the meantime, Prof. Müller had discussed the subject elaborately in the preface to the fourth volume of his edition of the Rig-Veda. The appearance of these two essays called my attention anew to the subject, and revived my intention of setting forth my own views in relation to it. On taking up the subject again, I have the advantage of what these eminent scholars have written, and I have permitted their arguments, I think, to modify my own views in reference to whatever points truth required me to do so.

Another fact should not pass unnoticed in this connection: M. Biot, in his review of the translation of the Sûrya-Siddhânta, published in the Journal des Savants, having made numerous unfavorable criticisms on parts of the work—which were handsomely, and, in general, justly replied to by Prof. Whitney—was quite severe on my closing note, and especially on the fact of my continued adherence to the opinion that the *nakshatras* had their origin in India. As if there was something unreasonable in my not being convinced by his arguments in favor of their Chinese origin—to which Prof. Whitney seemed to have yielded. His style of remark seemed to be rather overbearing and unwarrantably condemnatory; and the fact that I had merely stated my opinion, without giving the reasons in detail, seemed to strike his mind much to my disadvantage—it being regarded by him as evidence that I was unreasonably holding on to preconceived opinions which had been shown by him to be untenable, by unanswerable arguments, which I had refused to consider. Here, then, is an additional reason why I wish to express my views more fully.

Of the conclusions to which the four distinguished scholars before noticed, who have written on the subject, have arrived, those of Prof. Müller accord most nearly with my own, although, in his arguments to support his views, he has laid himself open to criticism—which criticism he has received at the hand of Prof. Whitney. Prof. Müller strongly maintains the Hindu origin of the system, and, while some of his reasoning is open to objection, other parts are valid. And here I may be permitted to remark that the essential arguments, on which I rely to show the probable, if not certain origin of the *nakshatra* system in India, I had written out before I saw the essay of Prof. Müller or Prof. Whitney's criticism upon it; yet I am free to acknowledge that his facts and reasoning have contributed, as I think, to enlarge and render more definite my views of the subject.

Prof. Whitney, after occupying some seventy pages in criticising the views and arguments of Weber and Biot, sums up his own conclusions in the following language:

"This is the array of probabilities upon which I base my suspicion that the Hindus did not, after all, originate the primitive system of lunar asterisms represented to us by the *nakshatras*, the *manâzil*, and the *sieu*. That it is but scanty, I freely admit: it is not of a character to compel belief, and I can quarrel with neither the candor nor the good sense of any one who shall refuse to be moved by it. I only claim that it is sufficient to prevent us from maintaining with confidence and dogmatism the derivation from India, either directly or indirectly, of the *manâzil* and the *sieu*, and to lead us to look with expectation rather than with incredulity for the appearance of evidence which shall show some central or western Asiatic race to have been the inventors of the lunar zodiac."

This is both courteous and liberal. Yet I cannot but express a little surprise at the rather indefinite position which the language implies. For, after reading the severe criticisms which Prof. Whitney offered on the arguments of Prof. Weber, who maintains the mediate Hindu origin of the systems—after having read his emphatic declaration in reference to the arguments of Prof. Müller, who maintains the absolute Hindu origin of the system, that they "are untenable"—after reading very numerous expressions scattered all along through what he has written on the subject, implying a belief more or less definite in the superior claims to originality of the *sieu* on the one hand or of the *manâzil* on the other—I say, after reading all this, I was expecting a stronger word than "suspicion" with which to characterize his conclusion; I was expecting a definite utterance of disbelief in the Hindu origin of the system, with perhaps a formal recognition of the claims of the Chinese or Arabians to the honor of having originated this division of the zodiac. But I do not attempt to make out a case of inconsistency. I accept the final declaration of Prof. Whitney, as above quoted, as expressing his belief. And I am glad that there is no more radical difference between his position and my own.

My position in reference to the subject under discussion may be indicated by the following theses:

The lunar division of the zodiac represented by the *nakshatras* had its origin in India.

The *sieu* of the Chinese have no genetic relation with the Indian *nakshatras*.

The *manâzil* of the Arabians were directly derived from India.

In support of these propositions, my arguments are based upon the following facts and considerations: viz., the indisputable documentary evidence of the existence of astronomical discovery, knowledge, and culture in India, which involved the recognition and use of the *nakshatra* system, as early as from the fourteenth to the twelfth century before Christ; the absence of reliable evidence of the existence of the system either in China or Arabia or any other country at that early date or for some centuries after; some resemblances and discrepancies between the systems as now found in the three countries respectively; the state of astronomical and other knowledge in the countries named, and the evident course of communication of knowledge and influence between different nations at that early time.

The existence of astronomical discovery, knowledge, and culture in India, as early as the fourteenth, thirteenth, or twelfth century before Christ, is proved mainly by the recorded position of the solstitial and equinoctial points, found in fragments of ancient-astronomical tracts that have been preserved, which can only be referred to that early date. The facts in relation to this point are briefly as follows:

At the time of the arrangement of the modern system of Hindu astronomy, as found in the Siddhântas—which took place not far from A. D. 500, when the Hindu sphere was made to commence at the first of Mesha or the beginning of the *nakshatra* Âçvina, that is, about 10′ west of the star ζ Piscium—it was a received astronomical fact, the universal belief with Hindu astronomers, that the equinoctial and solstitial points had fallen back about twenty-seven degrees from the places they once occupied, according to the first recorded positions in their ancient books. The details of the evidence of this I proceed to offer.

In the Graha Lâghava, the rule for calculating the amount of the precession for any given time is to deduct 444 from the years of the Hindu *çâka* era, i. e. of Çâlivâhana—and then reckon 1′ for a year, or at the rate of 24° for 1440 years. The commentator says that others make the precession to be 27° for 1800 years, which would give the yearly rate of 54″.

The author says that, in the *çâka* year 444, the *ayanânça* (precession) was nothing. This year corresponds with A. D. 522. This remark in an astronomical treatise is valuable, as giving us a native date* for the arrangement of the modern Hindu astronomy as found in the Siddhântas. The whole passage, with the commentary (though the work is comparatively modern), comes in as evidence of the general belief and understanding with Hindu astronomers, that, at the time when the Hindu sphere was fixed at the first of the sign Mesha and of the *nakshatra* Âçvina—viz. 444 of their era, or A. D. 522—the equinoxes had fallen back 24° or 27° from the points they occupied at the time of the first recorded observations found in their ancient books. Twenty-seven degrees of precession, at the rate of 1° for 72 years—the true rate, nearly—require 1944 years; which, reckoned back from A. D. 522, brings us to B. C. 1422, or, allowing the Hindu rate, of 54″ to the year, brings us to B. C. 1278.

In the Sûrya-Siddhânta, likewise, the rule for calculating the amount of the precession is evidently founded on the supposition that the equinoctial and solstitial points had receded 27° since the first recorded observations were made, which time was 1800 years before the commencement of the sphere was fixed at the point it occupied when that treatise was compiled. The rule is a very bungling one (for the explanations see Translation, ch. iii., vv. 9–12), and is evidently derived from the supposed fact that the equinoxes had moved eastward 27° in about 1800 years.

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* Bentley, from a calculation of the longitudes of the junction-stars of the *nakshatras* according to data given in the Siddhântas, makes this date to be A. D. 538; Prof. Whitney, by a similar calculation, makes it to come within the century preceding A. D. 560, or near A. D. 490 (see Translation of Sûrya-Siddhânta, p. 211, and Jour. Am. Or. Soc. vol. vi., p. 355). The native date above given is probably reliable.

Again, as a farther evidence tending to establish and prove this position, should be mentioned the fact that in the most ancient recorded lists of the Hindu *nakshatras* (in the texts of the Black Yajur-Veda and of the Atharva-Veda), Kṛttikâ, now the third, appears as the first. This indicates that since that ancient arrangement the equinoxes had fallen back two entire asterisms, or 26° 40′, at the time when the vernal equinox was found to coincide with the beginning of the asterism Âçvina. That is, the precession amounted to 27° wanting 20′. (Trans. Sûrya-Siddh., i. 27, note, and Prof. Whitney's essay, p. 84.) This amount of precession carries us back 1920 years, which, if we adopt the date given in the Graha Lâghava, above alluded to, viz., A. D. 522, as that of the time when the precession was nothing, and the fixation of the sphere took place, brings us to B. C. 1398. The value of this argument, of course, depends upon the supposition —against which I know of no evidence or probability—that the order of the *nakshatras* in that part of the system was the same as nearly 2000 years later, and that the beginning was at the vernal equinox.

We are thus brought by evidence which it is difficult to set aside or essentially impair, to the conclusion that there were records of astronomical observations, that involve the knowledge and use of the *nakshatra* system in India, as early as the thirteenth or fourteenth century before Christ. An allowance of one or two centuries of variation, on account of indefiniteness of the data and the uncertainty of the date of fixation of the modern Hindu sphere, makes no essential difference in the argument; and one or two hundred years are all that we are required to allow on this account.

There is definite evidence in fragmentary notices of ancient records of astronomical observations which carry us back nearly as far as the dates above mentioned; and these appear to indicate a continuance of attention to the science of astronomy, and progress in its cultivation, rather than a diminution from the time of the first observations.

The following are examples of this evidence:

Sir William Jones quotes the following from the Vârâhi-Sanhitâ, a work by Varâha-Mihira, who appears to have lived, as the extract shows, at the time the modern sphere was fixed—i. e., at the close of the fifth or beginning of the sixth century of our era:*

"Certainly the southern solstice was once in the middle of Âçleshâ, the northern in the first degree of Dhanishṭhâ, by what is recorded in former *shastras.* At present, one solstice is in the [first] degree of Karkaṭa [Cancer] and the other in the first of Makara [Capricorn]. That which is recorded not appearing, a change must have happened; and the proof arises from ocular demonstrations; that is, by observing the remote object and its mark at the rising or setting of the sun, or by the marks in a large graduated circle, of the shadow's ingress and egress." Asiatic Researches, vol. ii., p. 391.

According to this statement, the equinoctial and solstitial points had receded 23° 20′: i. e. the southern solstice had moved back through half of Âçleshâ, or 6° 40′, the whole of Pushya, or 13° 20′, and 3° 20′ of Punarvasû—as it was at the time in the 10th degree of this latter asterism—in all, 23° 20′. This, at the rate of 50″ a year, requires a period of 1680 years, which, reckoned from A. D. 499 (the date which Sir William Jones fixes upon), brings us back to B. C. 1181. Colebrooke coincides with Sir William Jones in the opinion that the record is a genuine one from Parâçara, and made when the colures were in the positions therein indicated. (Asiatic Researches, vol. ix., p. 357.)

But the following from the commentator of Varâha-Mihira, Bhaṭṭotpala, in reference to the above passage, is still more important. After saying that by "former *shastras*" are meant the books of Parâçara and other Munis, he cites the following from the Parâçara-Sanhitâ:

"The season of Çiçira is from the first of Dhanishṭhâ to the middle of Revatî; that of Vasanta from the middle of Revatî to the end of Rohiṇî; that of Grîshma from the beginning of Mṛigaçiras to the middle of Âçleshâ; that of Varsha from the middle of Âçleshâ to the end of Hasta; that of Çarad from the first of Chitrâ to the middle of Jyeshṭhâ; that of Hemanta from the middle of Jyeshṭhâ to the end of Çravaṇa." (Asiatic Researches, vol. ii., p. 393.)

This account of the six Indian seasons, each of which is measured by the time the sun occupies in passing through four lunar asterisms and a half, places the colures in the same position as the preceding extract from "former shastras" by Varâha-Mihira, showing that the work known as the Parâçara-Sanhitâ dates back to about B. C. 1181, and that its author, Parâçara, an ancient astronomer of great repute, lived at that time; and it is for this reason—that it identifies the time at which Parâçara lived—that Sir William Jones regards it as of so much importance in a historical point of view. But its astronomical or scientific importance is not less than its historical. It gives us an astronomical date, and indicates an important fact in the history of astronomy, which shows that at that time astronomy was receiving particular attention, and was cultivated on strictly scientific principles.

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* Sir William Jones puts the date at A. D. 499.

The same position of the colures is given in an ancient record, found in the *Jyotishas* of the Vedas. "To each *Veda* a treatise, under the title of *Jyotisha*, is annexed, which explains the adjustment of the calendar, for the purpose of fixing the proper periods for the performance of religious duties."* One of these tracts (which likewise contained another of the vedângas, the Çikshâ) was brought to me by a Brahman, who represented it as belonging to the Rig-Veda. I afterwards found the passage to which I refer quoted by both Colebrooke and Bentley, and still later by Müller. The most important part of this record is in these words:

"When the sun and moon ascend the sky together, being in the constellation over which the *Vasus* preside; then does the cycle begin, and the [season] *Mâgha*, and the [month] *Tapas*, and the bright [fortnight] and the northern path."†

"The sun and moon in the beginning of Dhanishṭhâ always turn towards the north, in the month of Mâgha, and in the beginning of Âçleshâ, towards the south in the month of Çrâvaṇa.

"In the northern course there is an increase of day and decrease of night amounting to a *prastha* of water; in the southern, both are reversed; in the course the increase and decrease are six *muhûrtas*."‡

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* Colebrooke's Essays, edition of Williams & Norgate, London, 1858, p. 65.

† This is Colebrooke's version. Prof. Müller translates the verse:

"When the sun and moon ascend the sky together, being in the constellation over which the Vasus preside, then does the (quinquennial) cycle begin, and the (month) Mâgha, and the warmth, and the bright (fortnight), for the path (of the sun) is north."

Of this verse I have given the versions of both Colebrooke and Müller, in order to notice the difference between these distinguished scholars in the translation of the word *Mâgha*. If my Sanskrit Lexicon is to be taken as authority, both translations are warranted. I may remark that my native Pandit considered the word as the name of the month Mâgha, agreeing with Prof. Müller. This point is of some importance, as showing the full recognition and naming of the Hindu months at that early period. And we have farther evidence in the verse preceding those the translation of which is given above, as given by Prof. Müller (the verse is not in the copy in my possession):

"They" (the vedângas) "teach the knowledge of time of the quinquennial lustrum, which begins with the light half of (the month) Mâgha and ends with the dark half of (the month) Pausha." I add parentheses enclosing the word *month*, as there is nothing in the original corresponding, though the meaning is implied.

‡ Colebrooke makes the following note on this passage: "I cannot, as yet, reconcile the time here stated. Its explanation appears to depend on the construction of the clepsydra, which I do not well understand; as the rule for its construction is obscure, and involves some difficulties which remain yet unsolved." (as above, p. 66.)

Colebrooke well styles this a "remarkable passage." It is certainly very valuable. It gives the position of the colures at the time of its record, such as to indicate an antiquity, as before observed, of about 1181 B. C.* And besides this, it proves conclusively that the Hindu months had been formed and named at that early period. This, it appears to me, is an important point in connection with our subject, as it has occasioned no small amount of discussion among our learned men, who have written respecting it. Bentley proved, to his own mind, that the Hindu months could not have been formed and named earlier than 1181 B. C., but thought they originated about that time. Prof. Müller represents Bentley as giving that as the period of their formation and receipt of their present names, and employs the fact as an argument for the antiquity of the *nakshatras* in India.† But Prof. Whitney, by elaborate calculations, proves that Bentley's reasoning is inconclusive, and that they might have been formed earlier; that in fact there is no limit in the nature of the principle of formation to the period within which they could have received their origin. But what is proved by the passage is—the names of two of the months being mentioned in it—that the system of months had been formed and names given to them, the same as they now bear, as early as the first part of the twelfth century B. C. But more than this is nearly the same as proved. The scientific cast of the record in the connection in which it is found proves that some matters in astronomy had been treated with approximate accuracy for centuries before. The position of the equinoxes had been noticed about 250 years before, which carries us back fully into the fifteenth century B. C. Now all this astronomical culture is connected with the *nakshatra* system. The names of the twenty-seven divisions of the zodiac are found in the *Jyotishas* of the *Rich* and *Yajush*, says Colebrooke, and in several places of the Vedas themselves.‡

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* Colebrooke (Essays, p. 67) distinctly states that the cardinal points of the ecliptic were in the positions indicated in the Jyotisha record above given in the 14th century B. C. This mistake must have arisen either from his reckoning the rate of the precession as less than one degree for every 72 years, or, as is more probable, from his confounding in his mind, at the time of making the remark, the fact that Krttikâ was formerly the first asterism, and hence the equinoctial points had receded back through two whole asterisms (26° 40′), since the first recorded observation. This, at 1° for 72 years, gives 1920 years, which, reckoned back from A. D. 522, brings us to B. C. 1398. If we take a later date for the fixation of the modern Hindu sphere—as A. D. 560, and later than this we cannot go—we must deduct so much from that date. And if we take A. D. 499, which many native astronomers (as shown by Jones, As. Res., vol. ii., pp. 392 and 398) regard as the date of the fixation of the present sphere—and which Prof. Whitney regards as possible (Translation of Sûrya-Siddhânta, p. 211, and Journ. Am. Or. Soc., vol. vi., p. 355), we have B. C. 1421.

This mistake of Colebrooke with its cause is so obvious that it seems a little strange that Prof. Müller and others should take so much space in refuting it. It simply needs to be corrected.

† Rig-Veda, vol. iv., p. xxxvii.

‡ As above, p. 67.

Prof. Whitney admits that the names of the deities presiding over the *nakshatras* are found in the early texts, but seems to imply, as I understand him, that he thinks these names were used before they were applied to the asterisms. But Colebrooke affirms that they were actually applied at this early period to denote the constellations themselves.* And in point of fact, it seems to be a natural conclusion, that the division of the zodiac into the twenty-seven or twenty-eight parts was first made, and then the names invented and applied to them. But, however this may be, the earliest astronomical records and traditions among the Hindus were evidently connected with this division.

The sum of results thus far reached is—that we have scientific documentary evidence that the lunar division of the zodiac into twenty-seven or twenty-eight equal parts known as the *nakshatras*, or lunar mansions, was known in India as early as the first part of the twelfth century before our era; and that, not as a semi-mythological fancy, but in connection with a system of true science based upon veritable astronomical discovery, invention, and observation; and the evidence is scarcely less conclusive that the system was known and in use in India 250 years earlier, or in the fifteenth century before Christ.†

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* As above, p. 67.

† Since writing the above, as my MS. was about to be sent to the press, I have received from Prof. Whitney his essay "On the Jyotisha Observation of the Place of the Colures, and the Date Derivable from it," published in the Journal of the Royal Asiatic Society of Great Britain and Ireland. It was dated June 1864. I have read this essay with great interest. Its object is to show the indefiniteness, unreliableness, and inaccuracy of the record, and its worthlessness for any use that can be made of it in founding a vedic chronology. The conclusion of Prof. Whitney is pretty fully shown in the following sentence (p. 327): "He who declares in favor of any one of the centuries between the eighth and eighteenth before Christ, as the probable epoch of the Jyotisha observation, does so at his peril, and must be prepared to support his opinion by more pertinent arguments than have yet been brought forward in defense of such a claim."

In regard to this conclusion or declaration, I simply remark, that, not so much from fear of the "peril" as from want of time and space, I do not attempt any reply to his arguments (the validity of many of the principal of which I do not admit); but I may admit the correctness of the conclusion implied in the sentence quoted, without essentially impairing my own argument as stated in the preceding pages. My object is not "to found a vedic chronology," but to show the antiquity of the knowledge and use of the *nakshatra* system in India. I can afford to deduct three or four centuries from the dates above given (so far as the indefiniteness shown by Prof. Whitney is concerned, they might be to be added), and still we have the clear irrefragable proof that this division of the zodiac was known and in use in India a thousand years before it was known in Arabia, and a number of centuries before it was known among the Chinese according to Weber, and about the same time it was completed there according to Biot. I say I may admit the conclusions of Prof. Whitney in regard to the point in question without essentially modifying my own arguments as stated above, when considered with reference to their object.

A strong point in Prof. Whitney's argument is that the Hindus had no instruments, that their astronomical observations were made by the unassisted eye alone. I think that what he has written on this point contains considerable unwarrantable assumption. But see below remarks in reference to this point.

Let us now turn to the other two nations, with each of which it has been claimed that the *nakshatra* system originated, viz. the Chinese and the Arabians. And first in regard to the Chinese. It will be sufficient for my purpose to note a very few facts in connection with the history and progress of astronomy among that people; my object being simply to inquire what evidence there is that the system was known in China as early as I have now proved it to have been in India.

And first, I remark that we have in the theory itself of those who advocate the Chinese origin of the system a refutation of its claims. According to M. Biot, the principal if not the only advocate of the Chinese origin—Prof. Whitney, who at first gave a modified assent to M. Biot's position, having subsequently withdrawn it—twenty-four of the twenty-eight stars called *sieu* were selected and named in the time of the Emperor Yao, in the year B. C. 2357, and four others were added some twelve centuries later, or about B. C. 1100. Our first impulse is to exclaim: what kind of division of the heavens, whether the reference is to the ecliptic or to the equator, is that which is only completed twelve hundred years after it was commenced! Again, this completion by adding four additional members to the system was certainly some centuries subsequent, in point of time, to the knowledge and use of the system in India. See above.

Again, secondly, the evidence which Biot alleges in support of his position that twenty-four of the *sieu* were named as early as B. C. 2357 is by no means satisfactory. He maintains this principally on the ground of the proximity of the stars in question to the equator of that time. But Prof. Whitney has shown that this is an arbitrary assumption: that the selection might as well indicate an equator hundreds of years after, or at least that that precise date is not indicated.* Prof. Whitney shows that the whole of Biot's reasoning in regard to this point is only from inferential grounds, plausibilities; that reliable evidence, either scientific or documentary, is almost entirely wanting. And Prof. Weber, in opposing the views of Biot, maintains that the *sieu* as a system of twenty-eight determinant stars is not traceable in Chinese literature further back than to about B. C. 250; thus setting aside the facts and reasoning of the French scholar as not supported. This fact is certainly a very damaging one to Biot's theory of the great antiquity of the system.

Thirdly, the Chinese system of *sieu* differs from the Hindu system of *nakshatras*, both in its structure and its object. In this system the determinant stars are at distances from each other varying from one half-degree to upwards of thirty degrees, making the intervening arcs or spaces, which are likewise called

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* See Prof. Whitney's criticism on this part of M. Biot's work in his Essay above quoted, pp. 34–38.

sieu, to vary among themselves in the same manner. Their use was to mark the meridional transits of the sun, moon, and stars. Whereas the Hindu *nakshatras* divide the zodiac into twenty-seven equal parts. It is true that the names are applied to groups of stars located within these spaces, but this is simply to mark the spaces or arcs themselves. And this seems to have been the application of the names from the first. In the earliest definite astronomical records, as cited above, in which some of the *nakshatras* are named, and the whole system implied, the names are given to the arcs of the zodiac. This was as early as about B. C. 1181.*

The object of the *nakshatra* system was to mark the progress of the sun, moon, and planets through the heavens. Such seems to be the import of the word *nakshatra* itself. Wilson, in his Sanskrit dictionary, gives the etymology as from *naksh*, 'to go,' and *atran* or *kshad*, 'a kind of root.' The last element of the word seems to be indefinite or not well known, but, from the first element, we have the clear idea of 'progress, path.'† I am the more particular in this matter, from the fact that Prof. Whitney's remark in relation to it seems adapted to mislead, or not to give the whole truth. He says: "No one of the general names for the asterisms (*nakshatra*, *bha*, *dhishṇya*), means literally anything more than 'star' or 'constellation:' their most ancient and usual appellation, *nakshatra*, is a word of doubtful etymology (it may be radically akin with *nakta*, nox, 'night'), but it is not infrequently met with in the Vedic writings, with the general signification of 'star' or 'group of stars': the moon is several times designated as 'sovereign of the *nakshatras*,' but evidently in no other sense than that in which we style her 'queen of night.'"‡ This remark may be true in regard to the two latter terms, and the later use of the first, *nakshatra*, but this evidently has a literal meaning in its composition something more than simply 'star' or 'group of stars.' The first element, *naksha*, clearly means 'progress, path' (i. e. the path of the sun, moon, and planets), and this is so modified by the last, *atran*, that the word becomes a proper appellative for one of the equal parts into which the whole 'path or course' of the heavenly bodies is divided; it would then naturally be applied to the groups of stars that should be selected to designate these parts respectively. According to this explanation, the term *nakshatra* had at first a meaning and application, both as definite and as general as the more modern phrase 'signs of the zodiac,' and 'zodiac.'

But the essential point in this connection is that the *nakshatra* system in India had from the first a strictly scientific structure and application, and this structure and application differ radically from those of the Chinese *sieu*. And M. Biot, while maintaining the derivation of the *nakshatra* system from the *sieu*, admits that this latter system was modified in its structure, and applied to a different use, as it passed into India. But this modification and different application make it another thing.

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* See above, p. 314.

† Prof. Müller (Rig-Veda, vol. iv., p. lxvi., note,) approves Yâska's derivation of *nakshatra* from the root *naksh*, having the sense 'to come, to approach.'

‡ Journ. Am. Or. Soc., vol. vi., p. 351; Trans. Sûrya-Siddh., p. 207.

Fourthly, there is another argument on which great reliance is placed in maintaining the derivation of the *nakshatras* from the *sieu*, or the reverse. This argument is based upon the fact of coincidence between the individual members of the two systems. I think this coincidence has been overestimated. In fact, I do not think it of any weight at all. The facts in the case are simply these: From a carefully prepared table by Prof. Whitney, it appears that there is an absolute coincidence between the determinant stars of only nine out of the twenty-eight—i. e. there are nine cases in which the *sieu* determinant star is the same as the junction-star in the corresponding *nakshatra;* in four other cases, the Chinese determinant is found among the group of stars embraced in the corresponding *nakshatra;* this number four might with a little allowance be extended to six or eight, so that we should have a full coincidence in nine out of the twenty-eight, or less than one-third, and a partial coincidence in eight more, or seventeen out of twenty-eight. This is certainly by no means remarkable, even on the supposition of the two systems being entirely independent of each other as to origin. And yet it is regarded by almost all the distinguished scholars who have written on the subject as settling the question as to the identity of origin of the two systems. For example, Prof. Whitney, after the table above alluded to, remarks: "after this exhibition of the concordances existing among the three systems"—he had included the *manâzil* of the Arabians—"it can, we apprehend, enter into the mind of no one to doubt that all have a common origin, and are but different forms of one and the same system;"* and Prof. Weber makes nearly the same remark. Other scholars share Prof. Whitney's confidence in regard to this conclusion. But I do not. And so far from it, I do not regard the coincidences as furnishing the basis of an argument of any weight whatever. Let us look at the circumstances of the case. Supposing two astronomers of the twelfth or twenty-third centuries B. C., or any other time, to undertake the division of the heavens into twenty-eight equal arcs or spaces, or to select through the whole circumference twenty-eight single stars or groups of stars, to mark these spaces, the one according to the principle of, and for the object of, the Chinese *sieu*, the other according to the principle of, and for the object of, the

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* Journ. Am. Or. Soc., vol. vi., p. 345; Trans. Sûrya-Siddh., p. 201.

Hindu *nakshatras* — each being ignorant of the other's work and design—let one be at Pekin and the other at Delhi or Ujjayinî, or one at Bagdad and the other at Paris, or any other two places of nearly equal latitude north or south—let them complete their work, and is it not highly probable—is it not nearly certain, that at least one-third of the stars selected would be identically the same, and should we not expect that one-half of the remainder would be nearly so; in short, should we not expect fully as great a number of exact and approximate coincidences as do actually exist between the *sieu* and the *nakshatras?* I certainly think so. In general, the most prominent stars would be selected by both astronomers, and this would secure exact coincidences in one-third of the cases, and an approximate coincidence in one-half of the remaining cases. And this is all that can be said respecting the coincidences in the two systems of which we are speaking.

But it is said that the coincidence in regard to twenty-seven or twenty-eight proves identity of origin, or at least affords a very strong support to such a theory. But is it so? In regard to this point, I remark, that twenty-seven (or twenty-eight) is the number that would naturally be selected if the object in view in the division was the one that is accomplished by the Hindu system. And twenty-four, the original of the Chinese system (according to Biot) is not an unnatural number, considering the object for which the system was formed. And the fact that the four, to make out the number twenty-eight of the *sieu*, were not added till after the system of twenty-eight *nakshatras* was known in India—probably not till centuries after—is conclusive against considering the coincidence in regard to the whole number as of any value as the basis of an argument.

But, fifthly, I cannot but mention, as adding not a little strength to my argument, the exceeding rudeness and vagueness and unscientific character of the astronomy of the Chinese, from its commencement to the time the Jesuit missionaries were placed at the head of their tribunal of mathematics. I use these terms, I think, with due deference to the opinions of M. Biot and other sinologues. M. Biot altogether overestimates the value of the materials which he used in forming his opinions, and I strongly suspect has failed in their correct interpretation in important respects. He at least draws important conclusions without adequate documentary evidence. Of doing this Prof. Whitney brings against him the direct accusation; and Prof. Weber does what amounts to the same, when he says that his (M. Biot's) views are "unceremoniously to be rejected as impossible."*

The materials from which M. Biot mainly drew his conclusions are the writings of the Romish missionaries, and more particularly those of M. Gaubil. Respecting these he uses the following language: "The writings of Gaubil which I have mentioned contain, in substance, all the documents necessary to reconstruct with entire certainty the ancient Chinese astronomy in its primitive simplicity and originality; but some precautions are necessary if we would employ these materials with safety."*

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* Essay of Prof. Whitney, p. 10.

Delambre, in his account of Chinese astronomy, makes use of the following language respecting the same author: "Here terminates the treatise on Chinese astronomy by M. Gaubil. We see that it contains only very vague notions, and no precepts which can give us the least light respecting the knowledge which the Chinese had of astronomy. And we can, without hazarding anything, say that astronomy was not born with the Chinese, notwithstanding the labors of so great a number of astronomers."

Delambre had, in his treatment of the subject, referred to the writings of other missionaries, but Gaubil is his prominent authority. Biot, likewise, has recourse to the writings of other persons, and to other documents; he even claims to have brought to light some original Chinese documents not known to the missionaries, but from these he does not deduce any new facts that should modify the conclusions to be drawn from the materials furnished by the missionaries. I am not aware that he claims for these other documents any great importance. M. Gaubil is his principal authority, as his language above quoted implies.

The statement of Delambre, as I think, fully sustains me in the use of the terms I employed in characterizing Chinese astronomy. The abilities and judgment of this astronomer are doubtless fully equal to those of Biot, and though the latter had the advantage of the advanced state of science over his brother of the preceding generation, yet his materials for forming an opinion on the subject in question were essentially the same.

My object in thus noticing the judgment of Delambre is to place in stronger light the ground of my suspicion—perhaps I should say opinion—that M. Biot and the missionaries who furnished his materials have misinterpreted—and hence misrepresented—not intentionally—the Chinese astronomical literature, when they date the formation of the system of the *sieu* as early as B.C. 2357.

But I must mention a few other facts leading to the same conclusion.

The emperor Yao sent four of his astronomers, one towards each of the cardinal points, to observe the stars that occupied the solstitial and equinoctial points respectively. They went, and, like obedient subjects, found the stars and reported—as if the observer, by going north, could better view the point of the summer solstice, and so of the rest!—and this fact is gravely recorded in the history of Chinese astronomy. From B. C. 2159 to B. C. 776, no mention is made of eclipses or of astronomy. In B. C. 687, mention is made of a night without clouds and without stars; towards midnight a shower of stars was seen to fall, which vanished as they approached the ether. The Chinese divided the circle into 365¼ degrees. They had nineteen rates for the precession of the equinoxes. Of the 460 eclipses recorded, only sixteen are verified, and not a dozen happened at the time as to year, month, and day designated. The first eclipse on the list is placed at B. C. 2159; but a part of the missionaries did not believe that the record relied on was designed to be that of an eclipse. Delambre did not believe the language designated an eclipse. Biot calculated the eclipse supposed to be intended, but found it could not have been visible in China.* In B. C. 164, the astronomer Tchang-Heng is said to have constructed armillary spheres, and a celestial globe and sphere, and to have made a catalogue of 2500 stars; but these are without latitude or declination.† It is said that their knowledge of astronomy was lost, when, in B. C. 213, the emperor Tsin-chi-hoang-ti caused all their books to be burned. M. Biot pretends that a few books on astronomy and astrology were preserved; but Prof. Weber thinks this fact—the destruction of the books—very damaging to Biot's theory of the antiquity of the *sieu.*

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* Précis de l'Histoire de l'Astronomie Chinoise, p. 6.

I mention these facts to show what was the character of the Chinese astronomical literature. And do they not prove that the astronomy of that people, down to the commencement of the Christian era, was vague, rude, and of an unscientific character, and that their records are not to be depended on? Not that the Chinese could not have originated the *sieu* system. This, at best, is only a bungling part of a bungling astronomical apparatus, a fit appendage to their rude astronomy. It is evidently Chinese in its origin.

In view of these facts, I cannot but think that Biot has much overestimated the character of the astronomy of the Chinese; and they afford ground for a strong suspicion that both he and the missionaries have, in important respects, misinterpreted the Chinese records, especially in regard to the antiquity of some of them, or that the Chinese themselves may have been guilty of forging and falsifying their records. And was there not an opportunity for this, when their astronomy was reformed in the second century of our era? And is there not evidence of this manipulation of materials and records from the account of the labors of the astronomers under the dynasty of the Han, a little later![*] Again, in this connection, we may note the fact of there being indisputable evidence that the Chinese astronomy was modified under a western influence, through communications from India, Arabia, and Greece, during the first centuries of our era. The names of the ascending and descending nodes are clearly from India, and the same is true of the names of some of the months. Biot says that the Greek zodiac was introduced into China in the time of Antoninus (Précis de l'Astron. Chinoise). Mohammedans were introduced into their tribunal of mathematics in the fourteenth century of our era. How very natural it would have been for their astronomers, having learned that the Hindus and Arabians had a division of the circle of the heavens into twenty-eight parts, to make the number of their *sieu* to correspond, by addition or curtailment? And is there not evidence that this manipulation of the number twenty-eight did actually take place, from Biot's own account of the *sieu* system?

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* Lettres Edifiantes, Tome xxvi., p. 97.

† Delambre, Ast. Anc., vol. i., p. 370.

Since writing the above paragraph, my eye fell on the following sentence in Prof. Whitney's essay—the facts and principles stated seem to be derived from Biot's works—which in a striking manner confirms this suspicion which I have long entertained: "As regards the appearance of the *sieu* in the Shi-King and the Shu-King, I am entirely of the opinion of Weber: that the mention in those works of stars or constellations which in part bear the same names with some of the defining-stars of the later system, and in part are identified with others of them by the commentators, does not in the least prove the subsistence of the system at the time; it only proves that the Chinese, industrious observers of the heavens as they seem to have been from a very early period, had already noticed and named some or all of the stars in the neighborhood of the ecliptic which are afterwards found to form a part of the series of the *sieu*." (p. 33.) Again: "The Chinese of a still earlier period, Biot would have us believe, had been in the habit of particularly observing the circumpolar stars, of noting their transits across the meridian, and of comparing therewith the transits of other stars. In the gradual improvement of their processes, they hit upon the plan of taking their fundamental stars nearer to the equator, for the sake of greater facility and accuracy of observation; but they were still so far under the dominion of their former method that they made choice of such new stars as were virtual representatives of the old ones, standing upon nearly the same circles of declination." (p. 36.) This, it is to be kept in mind, is Biot's theory.

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[*] The Han came to the throne B.C. 206. They collected again, and, for the most part, from oral tradition, the remains of Chinese literature. (See Prof. Müller, Rig-Veda, vol. iv., p. xliii.)

Respecting it, Prof. Whitney remarks: "It is here, again, first to be noted that all this is pure hypothesis on the part of M. Biot, and not in the least founded upon any record or tradition in the Chinese literature. No Chinese author has been shown to attempt to give the time or the manner of the first establishment of the series of *sieu*, or to explain the motives of their selection."

It is of no importance, as bearing on our argument, to determine whether this criticism of Biot's theory by Prof. Whitney is well founded or not. It probably is well founded. But whether it is or not, it clearly results, both from the theory and the criticism upon it, that there is no evidence of the *sieu* in a well defined system of twenty-eight determinant stars till a late period—perhaps some centuries after Christ—but that there is evidence of their changing nature and indefinite character, especially in regard to the determinant stars themselves and their number. Now when these facts are viewed in connection with some other prominent facts in the history of Chinese astronomy —e. g. the destruction of books on astronomy, B. C. 213, the reformation of astronomy in the second century after Christ, the astronomers under the Han collecting the remains of Chinese literature, and "mostly from tradition," their adopting from the Hindus the names of the nodes of the moon's orbit, and some of the Hindu months, the introduction of the Greek zodiac in the time of the Antonines (Biot, but no matter when), Mohammedans occupying their tribunal of mathematics in the fourteenth century, and the Jesuit missionaries a century or two later having the control of this tribunal, assisted of course by Chinese astronomers—in view of all this, have we not ground for more than a suspicion that the number twenty-eight is not Chinese, but from India or Arabia?

In view of all these facts and considerations, I am clear in the conclusion that the *sieu* of the Chinese and the *nakshatras* of the Hindus had no genetic relation to each other, and, if either was modified by the other, the modification was in this respect—that the number twenty-eight in the former was derived from the latter.

Let us now turn to the only other people with whom it has been claimed that the lunar zodiac might have originated, the Arabians.* Here a brief space will suffice to set forth the principal facts from which we must draw our conclusions. And it is almost sufficient to inquire, Where were Arabic literature and science at the time the first of the Siddhântas of the Hindus were compiled, about A.D. 500? It is true Abulpharaj, in his "History of the Dynasties," speaks of the Arabs as given to the cultivation of letters, and even of astronomy, so far as to observe the rising and setting of the stars;* yet may it not be said that astronomy among the Arabs cannot date back to a period before the rise of the Mohammedan religion? Delambre, in his great work, the History of Astronomy, does not treat of the astronomy of the Arabs in his volumes on ancient astronomy, but in that on the astronomy of the middle ages. And the first author he mentions is Abougiafar Almansor, who lived in the eighth century after Christ. This author is said to have studied law, philosophy, and principally astronomy. His great-grandson, Abdallah Almamoun, who reigned at Bagdad in A. D. 814, was farther advanced. He had a Persian preceptor, and sought for Hebrew, Syriac, and Greek books, and had them translated. The Syntaxis of Ptolemy was translated by Isaac ben Honain, in A. D. 817.

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* The evidences of a common origin of the *manâzil* with the *nakshatras* are so clear that I shall not discuss the points of coincidence and discrepancy between the two systems.

Again, we have clear historical proof that the Arabians received astronomical knowledge at this early period by direct communication from India. Thus Colebrooke, in the preface to his Hindu Algebra, states the following: "In the reign of the second Abbasside Khalif Almansûr... (A. D. 773) as is related in the preface to the astronomical tables of Ben-Al-Adami, published... A. D. 920, an Indian astronomer, well versed in the science which he professed, visited the court of the Khalif, bringing with him tables of the equations of planets according to the mean motions, with observations relative to both lunar and solar eclipses, and the ascension of the signs; taken, as he affirmed, from tables computed by an Indian prince, whose name, as the Arabian author writes it, was PHIGAR." (Colebrooke's Hindu Alg., p. lxiv.) That the Arabians were thoroughly imbued with a knowledge of the Hindu astronomy before they became acquainted with that of the Greeks, is evident from their translation of Ptolemy's Syntaxis. It is known that this great work first became known in Europe through the Arabic version. In the Latin translation of this version, the ascending node (Gr. ἀναβιβάζων σύνδεσμος) is called *nodus capitis*, 'node of the head,' and the descending node (Gr. καταβιβάζων σύνδεσμος), *nodus caudae*, 'node of the tail'—which are from Hindu appellations. (See Latin translation of the Almageste, B. iv., ch. 4; B. vi., ch. 7, et al.)

Another fact showing the belief of the Arabians themselves respecting their indebtedness, in matters of science, to the Hindus, should be mentioned. They ascribe the invention of the numerals, the nine digits, as they are called (the credit of whose invention is quite generally awarded to the Arabians), to the Hindus. "All the Arabic and Persian books of arithmetic ascribe the invention [of the numerals] to the Indians." (Strachey, on the early History of Algebra, As. Res., vol. xii., p. 184; see likewise Colebrooke's Hindu Algebra, pp. lii.–liii., where the same is shown from different authority. Strachey's article was published previously to that of Colebrooke.)

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* Delambre, Ast. du Moyen Age, p. 1.

We cannot, however, date the first knowledge of the lunar mansions—the *manâzil*—among the Arabians at the time of the visit of the Hindu astronomer to the Khalif Almansûr, A. D. 773, as stated by Colebrooke (see above), for they are mentioned in the Koran,* and Prof. Müller says the Bedouins had observed them long before the time of Mohammed.† On what authority he rests this last assertion, he does not inform us, or, if he has stated it, the statement has not fallen under my notice. But we may take such a fact, with a good degree of confidence, on his authority. And yet, it is so often the case that general indefinite declarations in regard to important facts, or supposed facts, are, on insufficient grounds, made by learned men, and obtain a current reception in literature, that I cannot but entertain grave suspicions in regard to them when the authority is not given, so that I can subject it to a thorough examination. And I really suspect that the words "long before" would be found, on strict scrutiny, not to be applicable to the real facts in the case. But, admitting the statement as correct, those who make it would not have the words cover a period of more than two, three, or four hundred years; so that, even on this admission, we have the well established fact, that the *nakshatra* system was known in India at least from twelve to fifteen hundred years before the *manâzil* were known among the Arabians.‡

We must, therefore, conclude with confidence, that the theory of the Arabian origin of the lunar zodiac, represented by the *nakshatras*, must be given up, there being absolutely no evidence in its favor.

But did not this system originate with some other Semitic people, from whom the Arabians received it? Such a supposition has been suggested as plausible. I dismiss this point by merely saying, when facts entitled to be regarded as evidence of such an origin shall have been produced, I shall be willing to give them their due weight. But at present, I know of no facts that afford any plausibility to the supposition. This division of the zodiac was known in India, or to the Sanskrit-speaking people on the confines of India, at least twelve centuries before Christ, and I am not aware of any evidence that can afford the ground of a suspicion that any Semitic people had a knowledge of the system till nearly or quite fifteen hundred years later.

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* Koran, x. 5; xxxvi. 39. See Müller, Rig-Veda, vol. iv., p. lxix. † Ib.

‡ Some three or four centuries must be deducted from this, if we admit the indefiniteness claimed by Prof. Whitney in regard to the Jyotisha record. See above, p. 317.

That Prof. Weber should adduce the fact of the occurrence of the words *mazzaloth* and *mazzaroth* in the Hebrew Bible (Job, xxxviii. 32, and 2 Kings, xxiii. 5) as the Hebrew equivalent of the Arabic *manâzil*, and base upon it an argument for the origin of the lunar division of the zodiac in western Asia, very much weakens my confidence in him as authority or guide in such a discussion.* For the connection of the words named with *manâzil* is far from being certain; their meaning, according to lexicons and versions, is different; and if the supposed identity could be made out, the fact would prove nothing to the point, unless there were some evidence that their application is the same; and such evidence, so far as I am aware, is entirely wanting.

But did not this division of the zodiac originate in some central point, with some other people, from whom it was communicated to both the Indians and Arabians? Such a supposition has often been suggested. It is mentioned probably more than half a score of times by Prof. Whitney in his rather voluminous writings on this subject; it is likewise suggested by Prof. Weber.

Prof. Whitney, in stating what are the true grounds for suspecting that India received her system of *nakshatras* from abroad, says that " by far the strongest of them is the appearance of the same system in the possession of so many other Asiatic nations, and in the case of the Chinese, at least, from so early a period. That the hypothesis of the origination in India is positively excluded by these facts I would by no means claim: I only assert that they are much more readily explainable by supposing that the institution was first invented and applied at a central point like Chaldea, the seat of empire, commerce, and culture which are known to have had wide-reaching connections and influence in every direction." (Essay, p. 66)

In support of this "suspicion," Prof. Whitney mentions three or four reasons: as that Hindu propagandism began, so far as we know, with Buddhism; the Arab and Chinese systems seem to be at several points in nearer agreement with the presumable original; the Hindus were not a practical people, "not of such habits of mind that we should expect to see arise among them an institution like the lunar zodiac, of so practical a bearing, founded upon faithful and long continued observations of the heavenly bodies, and intended for chronometrical uses;"† their failure to notice the lesser planets till the time they felt the influence of the Greek astronomy—implying that their first knowledge of these bodies was from the Greeks; and their having no instruments, all their observations of the heavens being by the unassisted eye.

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* See Prof. Whitney's essay "on the Views of Biot and Weber," p. 62. According to Prof. Whitney, Weber "mainly based his earlier rejection of Biot's views" on the occurrence of the words named in Hebrew.

† I thank Prof. Whitney for this characterization of the *nakshatra* system, and ask him in connection with it to keep in mind his own admission, and the admission of all Sanskrit scholars, that the existence of the system in India is traceable back to the Vedic period proper, and that almost " from the time of its first employment it shows no signs of change." (Essay, p. 67.)

I have not space for any adequate notice of these arguments and speculations. I merely remark that what I have said above in regard to the connection of the *nakshatras* and *sieu* is sufficient for that point. As to the practicalness of the Hindu people, were they not equal to, if not before, other nations of antiquity, especially in regard to astronomy, even admitting Prof. Whitney's claim of indefiniteness in regard to the date of the Jyotisha record? As to the Chaldean origin of the system, I ask for evidence: did not the Chaldean astronomy almost bodily, certainly the essential parts of it, pass over to the Greeks? and if the *nakshatra* system originated with the Chaldeans, or if they knew of it, is it not absolutely inexplicable that no traces are found of it in Grecian or Egyptian literature? That the Hindus did not notice the lesser planets till they received a knowledge of them from the Greeks, I simply do not believe; Bentley supposed he proved the planets were discovered in India in the fifteenth century B.C., and Prof. Müller endorses his argument and takes his facts; though I doubt the conclusiveness of Bentley's reasoning, the conditions of the problem are such that it cannot be satisfactorily disposed of by assumption, or a single declaration in a rounded period.

But the assertion that the Hindus, in the early period of their astronomical culture, had no instruments, and the use made of this assumption in the argument, I cannot let pass without more notice.* Prof. Whitney says:

"We have no reason whatever to believe that the Hindus who first employed the *nakshatras* possessed instruments, and had elaborated a system of observation of the heavens; their studies, beyond all question, were made with the eye alone." (Essay, p. 25.) And again, on the same page, speaking of the *nakshatras*, he says: "They were.... a series of twenty-seven or twenty-eight stellar groups, serving as basis for a division of the ecliptic into the same number of portions, as nearly equal as the unassisted eye could measure them;" and again, on p. 27, "that under them [the *nakshatras*] the planetary path was regarded as subdivided into twenty-seven equal portions, yet by the eye alone, and without any of that precision and fixedness which are given by the habit of observing with astronomical instruments."

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* Prof. Whitney attaches great importance, likewise, to the assumed want of instruments among the Hindus, in his essay "on the Jyotisha observation of the place of the colures, and the date derivable from it," published in the Journal of the Royal Asiatic Society of Great Britain and Ireland. In fact, the validity of almost all he says in his attempt to prove the indefiniteness of that date, and its worthlessness for any chronological purposes, depends on the assumed fact that the Hindus had no instruments. I must quote two or three sentences: "The ancient astronomy of India was, evidently, of a very rude character. It had neither the instruments nor the theoretical system of division of the circle necessarily implied by exact measurements. It knew no lesser parts of the ecliptic than the twenty-sevenths, or "portions" of the asterisms. Its observations were only such as could be made with the unassisted eye." (p. 322.) And then, as an inference from this assumption —as I term it—on the next page, he says: "If the Hindus themselves, in the ancient period, made an observation on the place of the colures which would have been an accurate one in the year 1360 B.C., no one should venture to draw from it any more precise conclusion than that it was probably made somewhere between 1800 and 1000 B.C." See other quotations and notices in reference to this point, above, in note on p. 317; and likewise below.

In reference to this language, I ask, with all soberness and due deference, what have we in it but pure gratuitous assumption? How does Prof. Whitney know that the Hindus had no instruments even as early as the time their system of twenty-seven asterisms was invented? In the sentence which follows the words I last quoted, he says, "that when, finally, a new and more exact astronomy was brought in from the West, ... a selection was made of one star in each group, to represent the group in the calculation of conjunction, and hence to be called its junction-star; ..... and that the time at which this definition was made .... must have been not far from A. D. 500." This date for the definition of the junction-stars of the *nakshatras* is undoubtedly correct, or nearly so, but the implication is that this was done under an influence from abroad—from Greece, as we must infer from other passages—and that till that time the Hindus were without astronomical instruments. It is to this implication, with the preceding direct assertion, that I object. It appears to me to be assumption without proper grounds. I of course cannot prove by documentary evidence that the Hindus had astronomical instruments at an early period in their astronomical studies, yet I cannot but think that there is as much in favor of such a supposition as there is against it. If the assumption that they had not instruments is granted, it certainly constitutes a powerful argument in favor of the non-originality in India of the *nakshatra* system—it is even an argument against the originality of any valuable part of the Hindu astronomy. But I do not admit it. For all the reasons that have been shown, we might, with equal plausibility, advance the opposite assumption, and say that the Hindus evidently had astronomical instruments at a very early period. This is to be inferred from the early knowledge and use among them of the lunar zodiac. This zodiac contemplates a division of the circle of the heavens into twenty-seven equal portions, each consisting of thirteen and one third degrees. This division could not have been made without an instrument; and then, again, it is evident that the practical value and use of this zodiac must depend on an instrument to aid the eye to mark the divisions, and we have evidence of the use of instruments in the early recorded observations: e. g. the place of the equinox was marked when it was at a point nearly 27° to the east of the commencement of the modern Hindu sphere; and in the first part of the twelfth century B. C., as is recorded in the Jyotishas attached to the Vedas, the southern solstice was observed to be in the beginning of the asterism Dhanishṭhâ, and the northern in the middle of Âçleshâ. This implies the knowledge and use of an instrument appropriate for such observations. In fact, we can scarcely conceive of their making any practical use of their zodiac—to say nothing of its invention—and of their accomplishing what it appears from their records they did accomplish, without instruments. The regulation of the sacrifices prescribed in the Vedas required a calendar, to establish which, with an accuracy necessary for practical use, required a knowledge of some instruments.*

I say, one might put forth, in such language, the theory or supposition that the Hindus had astronomical instruments from the commencement of their astronomical studies, and he would not be guilty of more groundless assumption than is involved in the opposite, which is asserted by Prof. Whitney.†

As I began the criticism on this point, I had in mind only the idea of making a humble protest against putting forth what I esteem a gratuitous assumption, and then basing upon it an important argument in reference to a great historical fact. But the more I reflect on the point, the more I am inclined to think that the supposition I have made as an offset to the opposite one made by Prof. Whitney is of some real positive value—is of value, because it has some real ground of support. For, in the first place, what implausibility is there in the supposition that, at the time the lunar zodiac was first known in India, there was a knowledge and use of appropriate instruments for its elaboration and application? Why may not an armillary sphere, somewhat like the instrument described in the modern Siddhântas, have been known even at that early day? Is it said that human art and ingenuity had not been at that time sufficiently developed for the invention of such an instrument? But can such an assertion be maintained? Was there not, before the flood, a Tubal Cain, who was an instructor of artificers in brass and iron? Did not Noah build the ark, which in some points has been a model in naval architecture to all subsequent times? Are there not ancient monuments in several countries which afford signs of a highly cultivated art in pre-historic times? And are there not on these monuments indications, yea proofs, of art-skill which we boasting moderns have never been able to surpass nor even equal? And why may there not have arisen some Hindu Hipparchus even as early as the fifteenth century B.C., who could invent an armillary sphere, and use it in constructing the lunar zodiac which seems to date from that early period? Surely the amount of ingenuity for inventing, and skill for making the instrument, is not so great as to preclude such a supposition. And after all, is it not a natural assumption that the mind which could conceive the lunar zodiac, represented in the *nakshatra* system, must, as a matter of course, have conceived of some apparatus like the armillary sphere to explain it and illustrate its use? Was not the construction of such a sphere a natural, if not necessary, consequence of the conception of its celestial prototype?

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* I learn from a remark of Prof. Whitney in his essay on the Jyotisha observation, before alluded to, that Dr. Haug, Superintendent of Sanskrit Studies in the Poona College, maintains the idea here advanced, that of an approximately accurate observation, in the preface to his translation of the Aitareya Brâhmaṇa.

† In regard to this point, it is not unimportant to remark, that the modern system of astronomy represented in the Siddhântas, compiled about A. D. 500, as all admit, is evidently a result of centuries of preceding culture. The Sûrya-Siddhânta, for example, is evidently the embodiment of astronomical facts and principles which required centuries for their discovery and application. A number of names of eminent astronomers are mentioned in modern Hindu works. The Brahmans have a tradition that their books were extensively destroyed about the time named. This helps to account for the fact that we have no more astronomical notices preceding the compilation of the modern systems.

But, lest I should be misrepresented, and accused of putting forth groundless assumptions and then basing upon them important arguments, I will state distinctly the results which I wish to have regarded as established by my remarks in relation to this point—the knowledge and use of instruments for astronomical discovery and culture in early periods of Hindu history. And first, the assumption that they had such instruments is not more groundless than the opposite one, that they had none; second, there is no implausibility in the supposition that they had astronomical instruments in early times; and third, the character of their astronomical records, the nature of the facts recorded, and general considerations necessarily connected with such an amount of astronomical knowledge and culture as they evidently possessed, render the supposition that they had appropriate instruments not only plausible but in a high degree probable. And the argumentative value I would have attached to these conclusions—respecting a knowledge of the planets and the use of instruments in India in early times—is simply this: they are fully an adequate offset to much that has been written by those whose views are somewhat opposed to the opinions which I hold in reference to Hindu astronomy; and, if the writings of these learned men may be taken, in regard to matter and style of argumentation, as a standard for the manner of treating the subject, these considerations are necessary in order to give a proper view of the matters under discussion. They are necessary, not only to place in proper light possible and plausible suppositions in relation to the subject, but likewise to do justice to the Hindus in regard to their abilities and actual achievements in the science of astronomy. I have no high idea of the Hindu astronomy, or of the practicalness of the Hindu mind; but I would have justice done them, out of a mere regard to the value of historical accuracy.

The result of my investigation, in my own mind, in relation to our subject, is this: From the evidences and materials for judging now available, we must conclude that the lunar zodiac represented in the *nakshatras* and *manâzil* originated with the Hindus or their immediate ancestry. The *sieu* of the Chinese have no genetical relation to them.

I say, from present evidence, for I hold my mind in an attitude to receive any additional light or evidence on the subject. But I look with incredulity, rather than expectation, for any important addition to our present materials for judging. For whence are such additional materials to be derived? Have not half a score of learned societies, under the titles of Asiatic, Oriental, Antiquarian, and Literary—Continental, British, and American—embracing all the antiquarian talent and interest of the age, been in this field of research for from an eighth to three-fourths of a century and more? And have they not in this time ransacked the whole extant literature of the world? Is there a monastery or nunnery or monk's cell, from the pillars of Hercules to the Caspian, which has not been explored and made to yield up its contents? Has not the literature of the Hindus been thoroughly examined, and the most valuable portions translated into European languages? And now, I ask, whence is this expected additional evidence to come? May we not say that, since the Vedas of the Hindus, the epic poems, and other ancient literature of these people, and especially the Siddhântas, have been thoroughly examined, and the principal and most important parts translated—other ancient literature having been subjected to nearly the same thorough research—may we not, in these circumstances, say that we have all the materials for judging the question of the origin of the lunar zodiac that we can ever expect to have? And when these materials are properly estimated, does not the evidence preponderate most overwhelmingly in favor of the Hindu origin of this appendage to astronomical science? I must say for myself, that, with this light, I cannot entertain even a suspicion of any other origin. And I look for important evidence of another origin with about the same hope and expectation as I do for the discovery of some new race, whose literature will modify essentially the present received history of any ancient people. If there is extant any undiscovered historical work of some Arabian, Teutonic, or Iranian Herodotus, let it be sought out, and we will hail it with joy, and permit it to modify our present opinions; and so, if there is any additional evidence in regard to the origin and history of the lunar zodiac, let it be produced, and we will give it its proper value; but until some additional evidence is brought forward, I must ascribe the origin of the lunar zodiac to the Hindus.